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Parenting programmes for incarcerated parents (Protocol)

McLaughlin K, Macdonald G, Livingstone N, Dempster M

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Parenting programmes for incarcerated parents

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

To assess the effectiveness of parenting programmes for improving parenting skills and outcomes for incarcerated parents and their children.

BACKGROUND

Description of the condition

There has been a substantial increase in incarceration rates worldwide. Recent figures indicate a 91% rise in the prison population in the UK between 1993 and 2014; the figure now stands at 84,372 ([Prison Reform Trust 2015](#)). In the USA over one and a half million people were held in prison at the end of 2014 ([Bureau of Justice Statistics 2015](#)). Consequently, the number of children affected by parental incarceration is significant ([Purvis 2011](#)). Approximately 65% of prisoners have children, and 7% of women who go into prison are pregnant ([Bureau of Justice Statistics 2004](#)). Annually, parents of over 200,000 children in the UK experience imprisonment; this is twice the number of children in care and six times the number of children subject to child protection plans ([Burry 2012](#)). The implications of rising incarceration rates reach far beyond the individual to the children, family and community ([Murray 2012](#)).

Incarcerated parents

Incarcerated parents may experience many life adversities before entering prison. Mental health problems are common in prisoners, many of whom experience more than one mental illness. A UK study revealed that 26% of women and 16% of men reported that they had received treatment for a mental health problem in the year before custody ([Prison Reform Trust 2013](#)). According to the US Bureau of Justice statistics, an estimated 56% of state prisoners and 45% of federal prisoners have a mental health problem ([Bureau of Justice 2006](#)). Incarcerated parents often report having little education; in 2013, a survey of prisoners in UK prisons found that 47% had no qualifications, compared with 15% of the working age general population ([Prison Reform Trust 2013](#)). Further, 53% of women and 27% of men in prison report having experienced sexual, emotional, or physical abuse as a child ([Prison Reform Trust 2013](#)).

Parents who are incarcerated often experience stress, related to being separated from their children. Mothers in prison who reported higher levels of parenting stress were more likely to suffer from depression and anxiety ([Houck 2002](#)). Higher stress levels appear

to be associated with an increased risk of violating prison rules and regulations, suggesting that those who experience parenting stress also have more difficulty adjusting to prison life (Loper 2006). Research indicates that incarcerated fathers feel powerless and personally devalued, and have lowered self-esteem (Purvis 2011). Skar 2015 found that incarcerated fathers, even after engaging in a parenting programme, suffered poor mental health and psychosocial functioning; the authors believed that this was due to an increased awareness of the negative consequences of being incarcerated on their child, and subsequent feelings of guilt.

Children of incarcerated parents

Children of incarcerated parents have been described as “among the riskiest of the high risk children in [the USA]” (Myers 1999, p 11). One of the biggest concerns is the increased risk of emotional and behavioural problems amongst these children, including mental health problems (Turanovic 2015). This may not be directly attributable to incarceration per se, as children whose parents are sent to prison are more likely than others to have been exposed to such adverse events as parental substance abuse, parental mental ill health, domestic abuse and poverty (Johnston 2012; Shlafer 2012). Further, living in a household with a criminally-involved parent can increase children’s risk of exposure to such adversities as sexual or physical violence, witnessing the arrest of a parent, out-of-home placement and - longer term - more punitive sentences if brought before the court (McCrudden 2014; Rodriguez 2009). These experiences may account for the increased risk of social, emotional and behavioural difficulties encountered amongst children whose parents have been imprisoned (Murray 2012; Tasca 2014; Wildeman 2014).

Nonetheless, parental incarceration can set in motion a cascade of negative consequences (Murphey 2015), beginning with adverse effects on parent-child contact and attachment. Maternal incarceration may be especially hard on a child, given that mothers are most likely to be the primary caregivers (Murphey 2015; Wildeman 2014). Although babies and children are sometimes allowed to reside with their mothers in prison (depending on jurisdiction), the large number of mothers who are incarcerated limits the possibility of this being an option for all and, after a certain age, the prison environment is deemed unsuitable for children (O’Malley 2016). For older children or children unable to remain with their mother, the care they receive may be adversely affected, as they may experience changes in caregiver or in the quality of care provided. Visiting a parent in prison can also be a very stressful experience. It is not surprising that under these circumstances children may become sad, anxious and afraid, and worry incessantly about their parent’s wellbeing and safety in prison (Gilham 2012).

As well as mental health risks, children of an incarcerated parent are more prone to cognitive delays and difficulties in school (Eddy 2010). They may be more likely to experience lower educational achievement and have an increased risk of truancy and

school exclusion (Morgan 2013). Research also suggests that maternal incarceration decreases teachers’ expectations of children, which is especially problematic at a time when children need as much support and encouragement as possible (Dallaire 2010). As the authors of Murphey 2015 note, the social stigma associated with having an incarcerated parent often brings disapproval, and is generally not supported by society. However, some research has revealed that maternal incarceration has no effect on children’s test scores and can actually increase their attendance and retention in school (Cho 2009). Thus, in some cases, especially when children are being exposed to harmful situations or neglect, maternal incarceration may lead to improved outcomes in some aspects of their lives (Turanovic 2012). For example, using data from the Fragile Families and Wellbeing study, Wildeman 2014 found that maternal incarceration was associated with less aggressive behaviour, less attention problems and less internalising and externalising problems, at least for white children, as reported by their caregiver. Thus, in some cases, maternal incarceration may improve child well-being.

Description of the intervention

Standard parenting programmes are usually short-term interventions aimed at helping parents improve their parenting, their communication and their relationship with their children (Barlow 2011). Most also aim to increase parental skills, knowledge and understanding of their child, and some seek more specifically to improve child emotional and behavioural problems via improvement of parenting behaviours. Programmes usually last for between 8 and 12 weeks, and deploy a range of techniques such as discussion, role play, video vignettes and homework (Barlow 2011). Many are manualised, and most are delivered to parents in groups in a range of hospital or community-based settings. They are underpinned by a number of theoretical approaches, including social learning theory, attachment theory, family systems theory and ecological theory.

Programmes designed for incarcerated parents differ from other programmes in two significant ways. In addition to focusing on the development of parenting skills, they need to deal with the challenge of parenting within the context of incarceration, and provide parents with the skills and strategies needed for positive engagement with their children, families and communities after release. This means addressing those factors that might threaten successful reintegration. Additional topics critical for this population might include identifying and avoiding abusive or negative relationships, staying away from drug and alcohol misuse, dealing with past trauma, and life development skills, such as job skills training and gaining employment (Kjellstrand 2012). As the authors of Kjellstrand 2012 note, these parents may only be able to parent effectively when these essential health and safety needs are addressed, and thus generic parenting programmes that focus

solely on parenting skills without addressing such particular issues may be of little benefit.

How the intervention might work

Many men and women who become incarcerated may also have experienced much childhood adversity, for example, violence in the home, substance abuse and physical or sexual abuse (Purvis 2011). Many may have lacked positive adult role models during their developmental years and, as Purvis 2011 states, these early experiences will inevitably shape their own parenting style in later life. Hence, there is a real need for incarcerated parents to be offered education and training in effective parenting, given that most lack the knowledge necessary to demonstrate positive parenting. Research has indicated that parenting programmes can be successful in helping parents develop their skills and resources, which, in turn, help them to parent more effectively (Wilson 2011). Components within the programmes can enhance parental knowledge of child development and positive parenting styles, which, in turn, can lead to enhanced parenting skills and parental self-esteem (Wilson 2011). Parenting programmes that improve the mental health of parents and their ability to regulate their emotions may also help parents in their parenting role (Barlow 2011). This seems particularly pertinent to incarcerated parents, given their risk of mental health and self-regulation difficulties. Loper 2006 cites the example of a mother who waits patiently for a visit from her children, only to have an uncontrollable anger display during the visit, leaving her wracked with guilt and the children not keen to visit again. Because these visits, whilst very beneficial, are so emotionally charged, prisoners would benefit from the opportunity to deal with past trauma and learn strategies that enable them to handle their own emotions and equip them to deal with the stresses they face more effectively (Loper 2006). Research also suggests that men's attitudes and ideas about their relationship with their partners and children may be unrealistic and unclear, which can lead to conflict within the home (Day 2005; Purvis 2011). Parenting programmes provide the opportunity, within a safe environment, for fathers to explore their beliefs and attitudes towards issues such as discipline, affection and family roles, which can support their reintegration back into the family upon release (Purvis 2011). It is also possible that promoting healthy parent-child relationships and responsive parenting may help break the cycle of intergenerational incarceration and improve the health and well-being of their children (Newman 2011). It should be acknowledged here that whilst all of these components are important, the existing evidence base is somewhat limited, and thus it is difficult to know how many studies actually measure these issues. Similarly, most general parenting programmes assume that the parents attending have at least some contact with their children and thus have an opportunity to 'practice' the skills being taught, which may not be the case for incarcerated parents.

Why it is important to do this review

The number of parenting programmes delivered to incarcerated parents has increased over recent years, and existing programmes designed for parents living in the community are often delivered to parents in prison, without modification (Eddy 2008). It is not clear what kinds of programme might be most effective for this population, and what changes, if any, are required to community-based parenting programmes to render them suitable for incarcerated parents (Purvis 2011). Hoffman 2010 has revealed that many programmes are being implemented without any knowledge of their impact as only a few institutions conduct evaluations. Indeed, we do not yet know whether parenting programmes for incarcerated parents have the potential to generate positive outcomes, or whether a universal, prison-specific intervention can meet the needs of this particular population (Kjellstrand 2012), given that prisoners are not a homogenous group (McCrudden 2014). Loper 2006 questions whether there are specific circumstances in which parenting programmes should not be used, as they may be harmful to the participants. For example, efforts to reconnect parents with their children may have a negative impact on the child if the parent does not consistently maintain contact with their child. Loper 2006 also cites the example of prisoners who have a severe personality disorder, and who may use the relationship with their children for self-gratification or manipulative purposes, which may not be beneficial to the child. Programme variation also raises a question about whether different types of programme are required for incarcerated mothers compared with incarcerated fathers.

This review aims to pull together the available evidence, to find out what, if anything, works, with whom and how, for this particularly vulnerable population and their children.

OBJECTIVES

To assess the effectiveness of parenting programmes for improving parenting skills and outcomes for incarcerated parents and their children.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled trials (RCTs) and quasi-RCTs (where participants have been allocated to a group via methods that are not truly random, such as alternate allocation).

Types of participants

Both mothers and fathers of any age, serving any prison sentence, who have at least one child/dependent under 18 years of age (living inside or outside the prison). The prison sentence can be for all forms of crime, both major and minor, and can be of any duration. Thus, for the purpose of studies based in the USA, we will include participants in both jails and prisons.

Types of interventions

Any face-to-face (either individual or group-based) or non face-to-face parenting intervention with a core component of either parenting skills, parenting attitudes or behaviours, or child development or behaviour. The duration and intensity may vary, however, the intervention must be delivered in prison or jail by either professionals or non-professionals. We will exclude programmes delivered via the internet or any other mode of delivery. Interventions will be compared to an inactive control intervention (for example, wait-list or no treatment) or an active control intervention (for example, at least one other psychological intervention or treatment as usual).

Types of outcome measures

Primary and secondary outcomes can be both parental and child outcomes.

Rating scales

We will include all relevant studies in the review irrespective of the tool used to measure the outcome, or whether the measured outcome data are reported in a 'usable' way. Data from the included studies will only be incorporated into the meta-analysis if a description of the scale and scoring information is available. If this information cannot be obtained, the data from these studies will not be incorporated into the meta-analyses, and details of the outcome measures will be presented as an 'Additional table'. Examples of measures that we anticipate finding are included after each outcome listed below.

Primary outcomes

1. *Parenting behaviours and skills (knowledge of child development, knowledge or visible improvement (or both) of appropriate parenting skills, knowledge or visible improvement (or both) of appropriate parenting attitudes/behaviours/practices), as measured by, for example, the Adult Adolescent Parenting Inventory (AAPI; [Bavolek 1984](#)); the Porter Parental Acceptance Scale (PPAS; [Porter 1954](#)); and the Child Behaviour Management Survey (CBMS; [Showers 1991](#)). These can be measured both inside the prison and after release.
2. *Adverse events (situations whereby an estranged parent, after participating in the programme, reconnects with his/her

child only to lose interest and become unreliable again, shortly after re-establishing the relationship, or situations where parents (particularly those with severe personality disorders) may use the relationship with the child for their own benefit, assuming that it would create a favourable impression for their parole board, whilst not having any real interest in the child's welfare or wellbeing). These may be assessed by personality measures such as the NEO (Neuroticism, Extraversion, Openness to Experience) Personality Inventory - Revised (NEO-PI-R; [McCrae 2004](#)).

Secondary outcomes

1. *Parental psychosocial health (stress, depression, anxiety, self-esteem, guilt, quality of life), as measured by, for example, the General Health Questionnaire (GHQ; [Goldberg 1972](#)), Parenting Stress Index (PSI; [Abidin 1995](#)), and the Beck Depression Inventory (BDI; [Beck 1996](#)).
2. *Parental recidivism rates held as government records.
3. *Child social, emotional or behavioural functioning (depression, anxiety, self-esteem, sadness, anger, internalising and externalising behaviour, feelings of alienation or abandonment (or both), delinquency), as measured by, for example, the Child Behaviour Checklist (CBCL; [Achenbach 2001](#)), the Strengths and Difficulties Questionnaire (SDQ; [Goodman 1997](#)), the Multidimensional Anxiety Scale for Children (MASC; [March 1997](#)), and the Risky Behaviour Protocol (RBP; [Conger 1994](#)).
4. Cognitive functioning or academic performance (educational attainment, school attendance, academic performance), as measured by, for example, school records or school grades, or both.
5. Parent and child or parent and co-parent relationship or contact (better communication between parent and child, better communication between parent and co-parent, improved relationship between parent and child, and enhanced parent and child contact or interaction), as assessed by, for example, the Parent-Child Relationship Questionnaire (PCRQ; [Furman 1995](#)), PSI ([Abidin 1995](#)), and the Maternal Sensitivity Scale ([Han 2002](#)).

Timing of outcome measurement

Data will be extracted post-intervention (up to one month following the delivery of the intervention). Where feasible, we will also collect short-term follow-up assessments (two to six months post-intervention) and long-term follow-up assessments (more than six months post-intervention), however, this is unlikely given the extent of the existing literature base.

'Summary of findings' table

We will use outcomes marked by an asterisk (*) to populate the 'Summary of findings' table.

Search methods for identification of studies

Electronic searches

We will identify relevant trials by searching the electronic databases listed below for all available years. We will not limit our searches by language or publication status, but will use a study methods filter to identify RCTs where appropriate.

1. Cochrane Central Register of Controlled Trials (CENTRAL; current issue) in the Cochrane Library, which includes the Cochrane Developmental Psychosocial and Learning Problems Group Specialised Register.
2. MEDLINE Ovid (1946 to current).
3. MEDLINE Epub Ahead of Print Ovid (current issue).
4. MEDLINE In-Process & Other Non-Indexed Citations Ovid (current issue).
5. Embase Ovid (1974 to current).
6. PsycINFO Ovid (1806 to current).
7. International Bibliography of Social Sciences EBSCOhost (1951 to current).
8. ERIC EBSCOhost (1966 to current).
9. CINAHL Plus EBSCOhost (Cumulative Index to Nursing and Allied Health Literature; 1937 to current).
10. Social Sciences Citation Index Web of Science (SSCI; 1970 to current).
11. Criminal Justice Abstracts EBSCOhost (all available years).
12. *Cochrane Database of Systematic Reviews* (CDSR; current issue) in the Cochrane Library.
13. Database of Abstracts of Reviews of Effects (DARE; current issue) in the Cochrane Library.
14. Epistemonikos (www.epistemonikos.org; all available years).
15. The Campbell Library (www.campbellcollaboration.org/lib; all available years).
16. SAMSHA's National Registry of Evidence-based Programs and Practices (www.samhsa.gov/nrepp; all available years).
17. ProQuest Dissertations & Theses UK and Ireland (all available years).
18. WorldCat (www.worldcat.org; limited to theses; all available years).
19. ClinicalTrials.gov (www.clinicaltrials.gov; all available years).
20. World Health Organization (WHO) International Clinical Trials Registry Platform (ICTRP; apps.who.int/trialsearch; all available years).

We will use the search strategy in [Appendix 1](#) to search MEDLINE and adapt it appropriately for all other databases.

Searching other resources

We will search the websites of relevant organisations and charities, including the following:

1. Barnardos (www.barnardos.org.uk);

2. Safeground (www.safeground.org.uk/impact-evidence/full-list-programme-evaluations);
3. Parenting Inside Out (www.parentinginsideout.org);
4. Action for Children (www.actionforchildren.org);
5. National Society for the Prevention of Cruelty to Children (www.nspcc.org.uk);
6. United Nations International Children's Emergency Fund (www.unicef.org);
7. Save the Children (resourcecentre.savethechildren.se);
8. The National Resource Centre on Children and Families of the Incarcerated, Rutgers University (nrccfi.camden.rutgers.edu); and
9. American Society for Criminology Conference Proceedings (www.asc41.com).

We will also search the reference lists of all included studies and any relevant reviews identified by the search. In addition, we will contact authors of included studies and other experts in the area, to enquire about potentially relevant unpublished trials and to obtain additional information not available in the published studies. We will also search Google Scholar.

Data collection and analysis

Selection of studies

Two review authors (KMCL and NL) will independently read the titles and abstracts identified in the searches. Studies that do not meet the inclusion criteria will be immediately discarded. Full copies of any potentially-relevant reports will be retrieved and both authors will independently determine whether studies should be included. All four authors will then come together to discuss this process and make decisions regarding the inclusion of any study rated as unclear. Reasons for exclusion will be recorded in the 'Characteristics of excluded studies' table. We will also provide a PRISMA flowchart illustrating our selection process ([Moher 2009](#)).

Data extraction and management

Data will be extracted independently by two review authors (KMCL and NL) and recorded on a data extraction form. This information will include:

- study design and method (RCTs, quasi-RCTs);
- participant characteristics (age, gender, etc.);
- intervention characteristics (method of delivery, number of sessions, etc.);
- outcomes and outcomes measures (any measures related to primary or secondary outcomes (see examples of measures reported under [Types of outcome measures](#)));
- outcomes measured versus outcomes reported;
- timing of data collection or follow-up;

- attrition and completion rates.

If disagreements occur, these will be discussed with reference to the full report and, if necessary, referred to the last author (MD). If more information is needed, we will contact the specific study investigators. Information on study design and implementation will be recorded in the 'Risk of bias' tables in the completed review. We will seek assistance from the Cochrane Developmental, Psychosocial and Learning Problems Group for articles published in languages other than English. We will also use the PROGRESS (place of residence; race/ethnicity/culture/language; occupation; gender/sex; religion; education; socioeconomic status; social capital) framework to ensure that our analysis and reporting encompasses an equity lens (Ueffing 2009).

Assessment of risk of bias in included studies

Two authors (KMCL and NL) will independently assess each study for risk of bias using the criteria recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011a). Each included study will be rated as low, high or unclear risk in the 'Risk of bias' table for each of the seven domains described below.

1. **Sequence generation.** The method used to generate the allocation sequence will be described in sufficient detail to enable an assessment of whether it should have produced comparable groups. An example of low risk of bias includes a computer random-number generator; an example of high risk of bias includes sequences generated by odd or even dates of birth; and an example of unclear risk of bias includes inadequate reporting.

2. **Allocation concealment.** The method used to conceal allocation will be described in sufficient detail to enable an assessment of whether allocation was sufficiently concealed before and during recruitment. An example of low risk of bias includes some central allocation (including telephone and web-based randomisation); an example of high risk of bias includes assignment envelopes used with appropriate safeguarding; and an example of unclear risk of bias includes inadequate reporting. An example of high risk of bias relevant to this population would be if the sequence is posted on a prison staff room wall.

3. **Blinding of participants and personnel.** Given the nature of the intervention, blinding of participants and those providing the intervention is not feasible. Therefore, the risk that this presents needs to be acknowledged.

4. **Blinding of outcome assessment.** The methods used to blind outcome assessors will be described in sufficient detail to ensure adequate assessment of the blinding process of those collecting the data. An example of low risk of bias includes blinding ensured with little chance of this being broken; an example of high risk of bias includes blinding with a strong likelihood that it could have been broken; and an example of unclear risk of bias includes inadequate reporting. An example of high risk of bias relevant to this review would be someone within

the prison informing the outcomes assessor as to whether participants are attending a parenting programme or not.

5. **Incomplete outcome data.** Data on attrition and exclusions, including the reasons behind these, will be recorded. An example of low risk of bias includes no missing outcome data; an example of high risk of bias includes the reason for missing outcome data likely to be related to the true outcome; and an example of unclear risk of bias includes insufficient reporting of attrition or exclusions to enable judgement. A relevant example of high risk of bias would be a participant not turning up to complete their outcome measurement due to inflexibility of the prison schedule.

6. **Selective outcome reporting.** An assessment will be made regarding the possibility of selective outcome reporting by study investigators. An example of low risk of bias includes where the study protocol is available and all outcomes specified have been reported; an example of high risk of bias includes instances whereby not all of the specified study outcomes have been reported; and an example of unclear risk of bias includes inadequate reporting. An example of high risk of bias would be when certain outcomes are not reported due to lack of statistical significance in the results.

7. **Other sources of bias.** Any potential source of bias (for example, early stopping of the trial) will be recorded. An example of low risk of bias includes studies that are free from other sources of bias; an example of high risk of bias includes where a study had an extreme baseline imbalance; and an example of unclear risk of bias includes insufficient information being provided to assess whether or not a risk of bias exists. A relevant example of high risk of bias would be where the programme is not being implemented with due adherence to the manual. We will resolve any disagreements about the 'Risk of bias' assessments by discussion.

Measures of treatment effect

Dichotomous outcome data

We will analyse dichotomous outcome data by calculating the odds ratio (OR) with 95% confidence interval (CI).

Continuous outcome data

We will analyse continuous outcome data by calculating the mean difference (MD) when studies use the same instrument to measure the same outcome, and the standardised mean difference (SMD) when multiple instruments are used to measure the same outcome. We will present both with 95% CIs.

Multiple outcomes

Where studies use multiple, interchangeable measures of the same construct at the same time point, we will calculate the mean SMD across the outcomes, in addition to the mean of their estimated variances. This avoids the need to select a single measure and the potential for inflated precision in the meta-analyses (studies that report on more outcome measures will not receive more weight in an analysis compared with those that report using only one measure).

Unit of analysis issues

Cluster-randomised trials

Should we identify cluster-randomised trials, we will adhere to the guidance outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2011). Cluster designs are susceptible to unit of analysis errors and artificially small P values (Deeks 2011). We will assume that study investigators will have controlled for this clustering effect in their results. Where the clustering effect has not been controlled for, we will request individual participant data to calculate an estimate of the intracluster correlation coefficient (ICC). This describes the variability in outcome within and between clusters (Donner 2001). If individual participant data are not available, we will attempt to find and use estimates of the ICC from similar studies. If we are unable to obtain estimates of the ICC from other studies, we will use arbitrary values and conduct sensitivity analyses (see [Sensitivity analysis](#)).

Studies with multiple treatment groups

If trials with more than two intervention groups are identified (multi-arm studies), we will refer to the guidance outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Deeks 2011). We will combine the results from all eligible intervention groups and compare them with the combined results across all eligible control groups to make single, pairwise comparisons. If this strategy causes difficulty when investigating potential sources of heterogeneity, we will analyse each intervention group separately, against a common control group. The sample size for the common comparator groups will be divided equally across each comparison to avoid double counting of participants. For dichotomous outcomes, the sample size and the number of people with events can be summed across groups. Means and standard deviations will be combined using methods outlined in the *Cochrane Handbook for Systematic Review of Interventions* for continuous and time-to-event outcomes (Deeks 2011). All decisions regarding these issues will be discussed in the review.

The review will also examine effects for mothers and fathers together and separately as part of the [Subgroup analysis and investigation of heterogeneity](#).

Cross-over trials

If we identify any relevant studies whereby participants receive both the control and the intervention but in a different order, we will include these in the review. However, we will only use data gathered up to the point of the first cross-over, to avoid problems associated with any carry-over effect.

Dealing with missing data

We will contact authors to follow up on any missing data (for example, missing outcome data, missing summary data, details of dropouts or any other relevant information). We will present information about missing data, in addition to attrition data, in the 'Risk of bias' table and we will also consider the impact, if any, that this may have on the results of the review. In the event that the study investigators used an intention-to-treat analysis (ITT) for both dichotomous and continuous data, we will use the results provided. In relation to missing data deemed to be 'missing at random', we will include the study data and analyse the data using an available case analysis. Where missing data are not 'missing at random', we will input the missing data under the assumption that the missing data are negative (for example, the parent-child relationship has deteriorated). We will explore the impact of this decision using [Sensitivity analysis](#). Where possible, if a study fails to provide the summary data to allow a meta-analysis, we will determine these using calculations provided in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011b).

Assessment of heterogeneity

We will assess clinical heterogeneity amongst studies by examining the variability in the participants (age, marital status), interventions (individual or group based) and outcomes (parenting behaviours and skills or parental recidivism rates). If we identify any unexpected variability, we will discuss it in full in the review. We will inspect methodological variability by examining the variability in study design and risk of bias amongst studies. Again, if any unexpected variability arises, we will discuss it in full in the review. Statistical heterogeneity will be assessed using the Chi² test and associated P value, the I² statistic, and by visual inspection of the forest plots. A P value lower than 0.10 for the Chi² test or an I² statistic of at least 50% will indicate statistical heterogeneity. An estimate of study variability will also be reported using Tau². In the event of significant, unexplained heterogeneity, we will interpret the results with caution.

Assessment of reporting biases

We aim to minimise reporting bias by contacting authors in the field and conducting comprehensive searches not limited by language or publication type. We will assess the possibility of publication bias and other small study effects using funnel plots of the

effect estimate from each study against the sample size of effect standard error when there is a sufficient number of studies. This is normally considered to be at least 10, as a smaller number would leave the power of the test too low to distinguish chance from real asymmetry. We will conduct Egger's test to test for funnel plot asymmetry (Egger 1997).

Data synthesis

Where possible, we will combine outcome measures from all relevant trials in a meta-analysis (that is, those with similar intervention characteristics, for example, mode of delivery, and similar settings such as group setting within prisons). We will apply both a fixed-effect model and a random-effects model and compare the results to determine the impact of statistical heterogeneity. We will calculate overall effects using inverse weighting methods in RevMan 2014. In instances where studies report an outcome as a dichotomous measure (for example, number of parents who re-offended) and others use a continuous measure of the construct (for example, recidivism rate of parents), we will convert results of the former from an OR to an SMD, provided we can assume that the underlying continuous measure has approximately a normal or logistic distribution, and that both are measuring the same construct. If this is not the case, we will perform two separate analyses. If meta-analysis is not deemed appropriate, we will provide narrative summaries of the studies. Given the varied evidence base, we will first map all studies into an appropriate intervention category, including, but not confined to the following:

1. Face-to-face (either individual or group based); and
2. Non face-to-face (either individual or group based).

When appropriate data from at least two relevant included RCTs or quasi-RCTs are available for any treatment comparison we will perform standard pairwise meta-analyses of the results.

'Summary of findings'

We will use the GRADE profiler Guideline Development Tool (GRADEpro GDT 2014) to import data from RevMan 2014 to create a 'Summary of findings' table using the outcomes highlighted in the Types of outcome measures section. These tables will provide outcome-specific information concerning the overall quality of the body of evidence of the studies included in the comparison, the magnitude of effect of the interventions examined, and the sum of available data on outcomes rated as relevant to parenting programmes for incarcerated parents.

We will employ the GRADE approach to assess the quality of the evidence as high, moderate, low or very low (Schunemann 2011a), depending on the presence of five criteria: (1) limitations in the design and implementation such as lack of allocation concealment, lack of blinding or large loss to follow-up; (2) indirectness of evidence, for example, where findings are restricted to indirect comparisons or where evidence comes from trials addressing a different question in terms of population, intervention, comparator

or outcomes; (3) unexplained heterogeneity or inconsistency of results, where studies have widely differing estimates of effect; (4) imprecision of results, that is, where studies have wide CIs due to few participants and few events; and (5) high probability of publication bias due to selective outcome reporting (Schunemann 2011b). Two review authors (KML and MD) will perform the assessment, resolving any disagreements by discussion.

We will also use the PROGRESS framework (Ueffing 2009), to ensure our analysis and reporting encompasses an equity lens. Further to this, we will include a section on clinical relevance and applicability of our findings.

Subgroup analysis and investigation of heterogeneity

We will explore possible sources of heterogeneity by conducting the following subgroup analyses.

1. Gender of parent - mothers only and fathers only.
2. Age of child/children - birth to 5 years of age, 5 to 11 years of age and 11 to 18 years of age.
3. Programme type - programmes designed specifically for incarcerated parents and generic parenting programmes.
4. Whether or not children live within the prisons.

We will only conduct subgroup analyses if we include 10 or more studies in a meta-analysis, as this number is necessary to ensure that the findings from these investigations of heterogeneity are useful (Schunemann 2011b). Difference in subgroups will be assessed using the formal statistical test for subgroup differences.

Sensitivity analysis

We will conduct a sensitivity analysis based on risk of bias, concentrating on factors such as sequence generation, allocation concealment and incomplete outcome reporting. Studies thought to be at high risk of bias for these domains will be removed to ascertain their effect on the pooled estimate. We will also use sensitivity analyses to examine the impact of different decisions (see Unit of analysis issues and Dealing with missing data) made throughout the process by the review authors, on the overall results. In addition, we intend to conduct a sensitivity analysis in relation to whether the comparison group was service as usual (SAU), no treatment or waiting list.

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* Indicates the major publication for the study

APPENDICES

Appendix I. Ovid MEDLINE Search Strategy

The following strategy includes the Cochrane highly sensitive search strategy for identifying RCTs in Ovid MEDLINE ([Lefebvre 2008](#)).

- 1 prisons/
- 2 prisoners/
- 3 Crime/
- 4 (borstal\$ or correctional or criminal\$ or custod\$ or detention\$ or felon\$ or gaol\$ or imprison\$ or incarcerat\$ or inmate\$ or jail\$ or penitentiary\$ or prison\$ or offender\$ or reform\$).tw,kf.
- 5 or/1-4
- 6 exp Parents/
- 7 Parenting/
- 8 exp Parent-Child Relations/
- 9 exp family relations/
- 10 exp maternal behavior/
- 11 maternal deprivation/
- 12 paternal behavior/
- 13 paternal deprivation/
- 14 (parent\$ or mother\$ or father\$ or maternal\$ or paternal\$ or famil\$).tw,kw.
- 15 or/6-14
- 16 education/
- 17 teaching/
- 18 (class\$ or coach\$ or curricul\$ or educat\$ or group\$ or intervention\$ or learn\$ or program\$ or support\$ or teach\$ or train\$ or workshop\$).tw,kf.
- 19 or/16-18
- 20 15 and 19
- 21 exp Parents/ed [Education]
- 22 20 or 21
- 23 randomized controlled trial.pt.
- 24 controlled clinical trial.pt.
- 25 randomi#ed.ab.
- 26 placebo\$.ab.
- 27 drug therapy.fs.
- 28 randomly.ab.
- 29 trial.ab.
- 30 groups.ab.
- 31 or/23-30
- 32 exp animals/ not humans.sh.
- 33 31 not 32
- 34 5 and 22 and 33

CONTRIBUTIONS OF AUTHORS

KMcL - wrote the protocol and NL, MD and GMD contributed to the drafting.

KMcL - designed the searches with expert advice from Margaret Anderson (Information Specialist).

DECLARATIONS OF INTEREST

Dr Katrina McLaughlin is a Lecturer (Education) in the School of Psychology, Queen's University Belfast. KMCL was awarded a Cochrane Fellowship to complete the review from the Health and Social Care Research and Development Division, Public Health Agency, Northern Ireland (HSC R&D, PHA). This covered training courses, support for travel, administrative costs and paid KMCL's salary for two days per week for two years. KMCL intends to submit another funding application to HSC R&D, PHA.

Professor Geraldine Macdonald is the Co-ordinating Editor for CDPLPG.

Dr Nuala Livingstone is an Editor for the CDPLPG and the Cochrane Editorial Unit.

Dr Martin Dempster is a Senior Lecturer in the School of Psychology, Queen's University Belfast.

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